though we have said nothing yet about the illustrations, they are also worthy of commendation as really illustrating the matter in hand, and being for the most part of excellent quality. But now we have the less pleasant duty of finding fault. Waterton had a strong prejudice against the use of scientific names. He tells us that the Salempenta is excellent eating; that you hear the voice of the Hannaquoi at early dawn; while such words as Conanacouchi, Labarri, and Karabimiti are continually used without any explanation of their meaning. In pursuance of his duty as editor Mr. Wood undertakes to clear up all these points, and to make the path easy both for the general reader and the scientific naturalist; and he does this by means of an "Explanatory Index," which occupies nearly one-third of the volume, and of which he says in his preface that he believes "there is not a single living creature or tree mentioned by Waterton concerning which more or less information cannot be found in this Index."

The index referred to does undoubtedly contain a great deal of useful and interesting information, but it is also full of the most extraordinary and misleading errors, which seem to show that Mr. Wood participates in his old friend's contempt for scientific names, since he evidently thinks accuracy in these names of little importance. First we have several completely obsolete names given, which the reader would in vain look for in any modern book on natural history; such as Champsa for Alligator, and Arapunga instead of Chasmorhynchus as the name of the bell-bird. Then we find misspelt or misplaced names; as *Derotypus coronatus* instead of *Deroptyus accipitrinus* for the name of the sun-parrot, and *Helias eurypyga* instead of *Eurypyga helias* for the sun-bittern. More important are the completely wrong identifications of species, or the mixing together of two quite different animals. The ant-thrushes are said to belong to the genus Pitta, which is eastern, whereas they form a peculiar American family, Formicariidæ. The feathers of the "wild turkey," a bird which does not exist in South America, are said to be used by the Indians of Demerara. The "hannaquoi," or motmot, is said to be named *Ortalida motmot*, and the description mixes up the real motmot (Momotus) and the gallinaceous Ortalida, saying that the eggs are blue and that the bird can be easily tamed and feeds with the poultry; which is certainly not true of the motmot, of which a figure is given, and which is a solitary forest bird whose eggs are white and which never walks on the ground. The "kurumanni" wax is said to be produced by a wild bee named *Ceroxylon audicola*, which is the name of the wax-palm of the Andes. The name of the "coral-snake" is given as *Tortrix scytale*, whereas the species belongs to a quite distinct family, being either an *Elaps* or a *Pliocerus;* while the deadly "labarri" snake is named *Elaps lemniscatus*, though, from the description Waterton gives, it is almost certainly a Craspedocephalus. The red grosbeak, which Waterton mentions as a rarity he was long in search for and gives a recognisable description of, is called *Cardinalis virginianus*, a bird not found in Demerara; whereas it is almost certainly the *Pitylus erythromelas*. The little tiger-bird is said to be a Tigrisoma or tiger-bittern; but Waterton's description shows it to be *Capito cayanensis*, a fruit-eating bird of a totally distinct family. The "yawaraciris" are said to be manakins of the genus Pipra; but the description in the text clearly points to the well-known "blue creepers" of the genus Cœreba. The jay of Guiana described by Waterton, and which Mr. Wood could not determine, is the *Cyanocorax cayanus*, while the "grand gobe-mouche," which is omitted from the index, is easily recognisable as the *Querula rubricollis*. Of the plant identifications I am not prepared to speak, except to remark that the cultivated pineapple is certainly not a species of *Pitcairnea*.

It is to be hoped that this delightful work will come to a second edition, and admit of these blemishes being removed. It would also be a great convenience if references were added to the explanatory index, to avoid the trouble of first going to the index proper and then back to the body of the work. These, however, are matters which, though important to the student who keeps the book for reference, will not much affect the enjoyment of the general reader; and I can therefore cordially recommend all who have not made the acquaintance of the "Wanderer" to do so in the pages of the present volume.

A. R. W.

OUR BOOK SHELF

Ueber ehemalige Strandlinien in anstehendem Fels in Norwegen. Dr. R. Lehmann. (Halle, 1879.)

PROBABLY no feature of Scandinavian geology has been more frequently discussed than the remarkable lines of terrace which have been traced along the slopes of the coast, even up into the far northern fjords. Certainly no stranger, even if ignorant of geology, can visit these regions without being impressed by the freshness and persistence of these "parallel roads," which wind in and out among the intricate navigation of strait and sound, islet and archipelago. From the time of Celsius downwards a continually increasing literature has been devoted to this subject, and now Dr. Lehmann, of the Realschule, in Halle, adds another essay to the pile. He discusses at length and rejects the theories of erosion by glaciers and by floating ice, and adopts that of breaker-action. But probably no exclusive theory is correct. Unquestionably Norway has been overridden by land-ice, scarped and notched by coast-ice, as well as cut into by tides and breakers. That the terraces mark lines of former sea-level seems so self-evident that it hardly deserves more than a simple mention of the fact. But when these lines were cut out of the rock and the land was a hundred feet or more lower than it is now, the coasts were doubtless cumbered with ice, and while the breakers were grinding out a platform from the solid rock, their work was probably expedited by drifting masses of floe-ice. Dr. Lehmann's pamphlet is useful for the collected references it contains to recent literature on the subject. But it is needlessly voluminous.

Die Lust an der Musik. Erklärt von H. Berg. B. Behr's Buchhandlung. (Berlin, 1879.)

THIS is a little pamphlet which we have perused with no small amount of disappointment. After a short chapter treating of the origin of music, in which the author merely recapitulates the theory expounded by Darwin long ago, we come to Chapter II., on the development of music, in which the author states very little that has not before been stated by Darwin, and particularly by Helmholtz, in his "Lehre von den Tonempfindungen." The principal chapter, viz., that on the effects of music, in which we expected to find the explanation promised in the title of the pamphlet, or at least the expression of some new ideas on the subject, occupies but four small

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pages, and contains merely a few illustrations of the capacity inherent in music of modulating the pleasant sensation it produces in the mind of man in a number of various ways. An appendix treats of the pleasure man derives from the aspect of colours, certain forms, and the beauty of the human body.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Spectrum of Brorsen's Comet

THE observations of Prof. Young on the present appearance of the spectrum of Brorsen's comet are of great interest, from the circumstance to which he refers in his letter in NATURE, vol. xix. p. 559, namely, that in 1868 I found the positions of the three bright bands of this comet not to agree with those of other comets which I showed to be coincident with the bright bands in the spectrum of flames containing carbon.

The care I bestowed upon the determination of the apparently anomalous character of the spectrum of Brorsen's comet in 1868 gives me great confidence in its approximate accuracy. I wish now to call attention to the fact that a spectrum apparently essentially similar to the peculiar one exhibited by Brorsen's comet in 1868, was observed at Dunecht by Lord Lindsay in the case of Comet C 1877 (Borelly's). It is remarkable that another comet, Comet B (Winnecke's) 1877, observed by Lord Lindsay on the same evening (May 6) presented the ordinary cometary spectrum.

Lord Lindsay's diagram in the *Monthly Notices* R.A.S. (vol. xxxvii. p. 431) of these two spectra agrees as nearly as can be expected in such observations with my diagram in the *Phil. Trans.*, 1868 (Pl. xxxiii.), contrasting the spectrum of Comet B, 1868, with that of Brorsen's comet.

It may be accepted, therefore, as beyond doubt that the unusual form of spectrum of Brorsen's comet in 1868 is occasionally presented by comets. The great interest of Prof. Young's observation lies in the information which it gives us that the *same comet* may present on one occasion one spectrum, and on another the other spectrum.

I regret that the special arrangement of my telescope for photographic work does not permit me to observe the spectrum of Brorsen's comet at its present appearance.

Upper Tulse Hill WILLIAM HUGGINS

The Migration of Birds

IN NATURE, vol. xix. p. 433, there is a notice of my paper "Ueber das Wandern der Vögel," to which I have somewhat to reply.

However agreeable it is to me that my views should be communicated to your readers, and however little I object to their being submitted to rigorous criticism, I must still also desire that this criticism be fair.

I believe it is due to differences of national customs that your reviewer has not quite satisfied this desire. We make, perhaps, in Germany a sharper distinction between a *scientific* treatise and a *popular* work than in England. Of the latter we do not require that it bring forth what is *new*, but only that it should give what it has to give in a clear and easily intelligible manner. Nor do we require completeness of such a work, or even a criticism of the scientific works on which it is based; indeed, it is generally left to the author how far to cite his sources of information and how far not. In the *scientific* treatise it is quite otherwise; here only that is of value which is new; the theme must be treated *exhaustively;* the sources must always be named and dealt with critically, &c.

Now my publication is a lecture, which was delivered *before a company of educated ladies and gentlemen, and so before mere laics,* and a year and a half afterwards was printed in Virchow and Holtzendorff's Collection of Popular Lectures. *It thus belongs unquestionably to the category of popular writings.*

For this reason your severe critic had no occasion to point out that in my lecture there is much that had been long known, that sources are named but rarely, and that no scientific criticism is exercised. That is quite a matter of course in a popular work, at least in Germany. Mr. Newton would have had much better right to feel surprised *that even any new ideas were contained in it.*

My original aim in this lecture was merely to make my hearers acquainted with the new facts and views on the migration of birds, as they have been established by Wallace, Middendorff, and especially by Palmén. As I followed the new facts theoretically to their consequences, there arose perhaps some new ideas, which I should be glad to find verified in the future.

It is further a matter of course that, notwithstanding the *popular* form of my work, I stand by all that I have said; but I must protest against being made responsible *for what I have not said!*

Thus, *e.g.*, I have nowhere said that I hold Palmén's routes of flight for "absolute truths." I am rather quite of Mr. Newton's opinion, that these routes are merely *inferred,* not *directly* observed, and therefore that they are to a certain extent "conjectural." In this sense, however, the routes of birds must ever remain comparatively "conjectural," unless one were to follow the birds in a balloon. But while "conjectural," Palmén's routes are yet inferred by a purely scientific method, and I doubt not that most of them will in the main be confirmed by further observations. Precisely in the application of this method lies Palmén's great merit, and it is only to be hoped that ornithologists will follow further in his footsteps, and correct his mistakes by accumulation of new facts. That Palmén's routes contain some errors I do not doubt; I should rather wonder if it were not so.

Little, however, comes of this with reference to the questions which are treated with special fulness in my lecture, *the origination of the instinct of migration, and the powers by which the bird reaches its distant goal.*

I have, further, nowhere said that birds fly over the sea at a height of 20,000 feet, but have merely cited the fact that birds have been seen at such height; with reference, of course, to explanation of their flight over the sea. I believe that birds, in flight over the sea, do not close their eyes, but exercise their keen eyesight as far as possible. Therewith, however, it is not said (as Mr. Newton imputes to me) that in *all* flights over the sea they always keep the land in sight.

I desist from adducing further misunderstandings by Mr. Newton, and come to what *I have actually said* and am minded to maintain.

In agreement with Palmén, I have expressed the opinion, *that migratory birds have no special sixth sense,* as Middendorff has assumed, but that *they find their way only with the help of their ordinary five senses.*

Mr. Newton seems to be of a different opinion. He does not say, indeed, whether he agrees with Middendorff, but he brings forward observations which appear incapable of harmony with my view.

First, there appear in New Zealand two species of cuckoo (*Chrysococcyx lucidus* and *Eudynamis taitensis*) which regularly fly some 1,000 miles' distance over the ocean. I believe with Mr. Newton that the birds cannot fly so high as to see at once New Zealand and the Norfolk or Kermadec Islands, though on the former is a hill of 1,000 feet. Likewise I will accept the case of *Charadrius pluvialis* as a regular guest of the Bermuda Islands, and a doubtful *Charadrius* species as regular guest of the Sandwich Islands. *All these observations are, indeed, still very imperfect,* inasmuch as it is not known whence the birds come nor whither they go; but so much seems certain, that they do *regularly* fly over large stretches of ocean in which are almost no islands or rocks, and which are so great that they must of course also fly by night.

What then? Are we therefore compelled to make the assumption, with Middendorff, of a sixth sense, which informs the bird which direction is north? Is there no simpler explanation of the fact? Obviously, we should only be warranted in accepting such a purely hypothetical sense, if it were clearly proved, that we could never get to understand the facts without it.

The question had already occupied me, before I knew of Mr. Newton's examples. I omitted it in my lecture, because it seemed to lead me further into the region of hypotheses than I considered I could answer for before my audience.

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